

of carbonic acid. It is accompanied by a molecular change which renders the resulting product soluble and diffusible. Assimilation is simply the absorption by the living tissue of the substances thus prepared, one of the chief processes which accompanies it being the reversion, by loss of water, of the glucose to the condition of cellulose, a substance isomeric but not isomorphic with starch. Intussusception, therefore, is a process which can only succeed digestion. No essential difference can, in fact, be maintained between the manner in which animals and plants digest their food. A. W. B.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Hygroscopic Seeds

I HAVE lately received an interesting letter from Fritz Müller, in St. Catherina, Brazil, on the subject of hygroscopic seeds. He tells me that in the highlands of the Uruguay he has succeeded in discovering more than a dozen grasses, as well as a species of geranium, whose awns are capable of hygroscopic torsion. He has been so kind as to send me specimens of the grass-seeds, and many of them appear to be as beautifully adapted as those of *Stipa*, *Avena*, &c., for penetrating the ground in the manner which I have elsewhere described.¹ The most curious among the specimens received are the seeds belonging to the genus *Aristida*. In one of these the awn is longitudinally divided into three fine tails, six or eight inches in length, each of which twists on its own axis when the seed is dried. These tails project in three directions, and more or less at right angles to the axis of the seed, and Fritz Müller states that they serve to hold it in an upright position with its lower end resting on the ground. The seed is pointed and barbed in the usual manner, and when it is made to rotate by the twisting of the awns, it evidently forms a most effectual boring-instrument, for Fritz Müller found many seeds which had penetrated the hard soil in which the parent plant was growing. Another species of *Aristida* is interesting to me, because it illustrates the explanation which I gave of the torsion of the awn of *Stipa*, namely, that each individual cell of which the awn is composed is capable of torsion, and their combined action results in the twisting of the whole awn. Now in this species of *Aristida*, each of the three tails into which the awn is divided is capable of torsion on its own axis, and as the seed dries they twist up into a perfect three-stranded rope, just as the component cells combine to produce the rope-like twist of the *Stipa* awn. And as the tails wind together and form the strands, the seed is made to rotate and thus bury itself in the ground.

Down, Beckenham, February 19

FRANCIS DARWIN

Mind and Matter

BUT for illness I would have made an earlier reply to Mr. Duncan's courteously-expressed objections (*NATURE*, vol. xv., p. 295) to my analysis (*NATURE*, vol. xv., p. 217) of his very ingenious "solution" (*NATURE*, vol. xv., p. 78). A general "mistake," and an "essential omission," are the charges against me. The mistake is in "regarding what was intended to solve a problem as intended to prove an alleged fact." "The alleged fact," he adds, "that consciousness depends on nervous organisation, I assumed to be a fact, and undertook to indicate *how* the dependence might be conceived, or regarded, to exist." He says that I clearly understood this "at starting." Where now is it that I "fell into the error?" His first step towards "clearing away difficulties in the way of our conceiving the relation of consciousness to matter," is to allege this fact: "It is no more difficult to conceive of matter being subjective than of spirit being subjective." This is a dogmatic statement about our powers of conceiving; no hint of help as to *how* we may conceive. We ordinarily conceive of "spirit"—the "ego," the "subject"—as susceptible to consciousness, or "subjective," because we (the ego) feel we are conscious; but it is "as easy" to conceive of a stone as susceptible to consciousness, *i.e.* subjective? To say it is, I called a *petitio prin-*

cipii, because it assumes that conceivability which has to be established. I used the word "probability" as involving conceivability; for can we intelligibly assume a probability without a conception of what that probability is? But Mr. Duncan contends that his position is "conceivable as a hypothesis, true or false." Unquestionably we may conceive some one stating any hypothesis—a stone feels, fire freezes—but to conceive one doing this is not to have a concept of any part of the operation as hypothesised, however we may attach a meaning to the terms as such. Again, if any hypothesis, true or false, is already conceivable, this fact cannot favour Mr. Duncan.

So far I have not been led "to mistake allegations of the conceivability of a notion for assumptions or intended proofs that the notion is true." To the next position, "How energy is related to matter, is no less mysterious than how subjectivity may be a property of matter," my objection was twofold: first, to the illogical form; second, to the argument itself. Mr. Duncan replies, "The parity of mystery was not intended to establish parity of probability as to facts, but merely parity of conceivability." Now what is *conceivable* in the known case? The *fact* of energy being related to matter. Next, what here is mysterious or *inconceivable*?—the *manner how* these are related. Finally, what is the parallel to establish? Mr. Duncan answers, "Not the parity of probability as to facts, but merely parity of conceivability." But the conceivability of *how* energy is related to matter equals zero, therefore, by parity of reasoning, the conceivability of how subjectivity is related to matter equals zero. I commented, therefore, on all that this argument supplied—a bare shadow of *probability*. My next objection to the position, "Energy may be divided, why not subjectivity?" is strictly categorical, and no flaw has been found in it, nor, intrinsically, in any of my objections, which have now been shown to apply to "conceivability." Of the omission, Mr. Duncan says:—"The essential part of my solution which indicated roughly the *modus* of the connection between matter and consciousness, and which dealt with the great difficulty of the question, How to account for the two aspects of matter, the conscious and the unconscious? has not been touched by Mr. Tupper." Because all this was based on the untenable ground that "subjectivity may be divided," I closed my analysis here; but will conclude with a few remarks on the ingenious and original parallels drawn by Mr. Duncan.

"As energy potential is rest, so subjectivity potential is unconsciousness. As kinetic energy is motion, so active subjectivity is consciousness." Now energy, both to the materialist and his opponent, is a hypothesis, not a phenomenon; and it is not legitimate to support one hypothesis by another.

Again, if subjectivity is defined "susceptibility to consciousness," some sub-definition of "susceptibility" is needed; for if non-innervated matter, as Mr. Duncan admits, is never conscious, then matter *in this form being non-susceptible to consciousness*, is by the definition non-subjective: a conclusion opposed to Mr. Duncan's "all matter is subjective or susceptible to consciousness," his qualification, that non-innervated matter is only "potentially subjective" not availing unless this term mean non-subjective, and leave us with the above contradiction. The expression "all forms of matter may, by innervation, be made susceptible," &c., would indeed carry the conclusion "all matter may be made subjective," but then subjectivity would be an accident, not a property of matter as defined by Mr. Duncan. Lastly, to the phenomenalist who would investigate, and not create, nature, matter, or a fancied common substance for the support of all phenomena, is perhaps the most unwarranted of all assumptions.

J. L. TUPPER

Atmospheric Currents

MR. CLEMENT LEY thinks (see his letter in *NATURE*, vol. xv p. 333) that if the earth's atmosphere contained no watery vapour, the great currents of atmospheric circulation would be quite unlike what they are. I think, on the contrary, it is as certain as the established truths of physical astronomy, that if there were no watery vapour the great currents, though not the storms and other temporary disturbances, would be nearly what they actually are.

All winds belonging to the great currents, though not local winds, form part of a system of circulation between the equatorial and the polar regions, which is caused by the difference of those regions in temperature. Equatorial air is constantly flowing towards the poles, and polar air towards the equator; the equatorial air brings the greater rotatory velocity of the equatorial

¹ *Trans. Linn. Soc.*, vol. i., part 3, p. 149, 1876.

regions into the higher latitudes, and the polar air brings the less rotatory velocity of the polar regions into the lower latitudes. The latter constitute the trade-winds, which move more slowly than the earth's rotation, and consequently appear as an atmospheric current from the east; the former constitute the "counter-trades," which move more rapidly than the earth's rotation, and appear as an atmospheric current from the west.

The centrifugal force of the "counter-trades," as they circle round the poles, is the cause of the polar depression of the barometer.

The law of reaction makes it impossible for the earth's rotation to be either accelerated or retarded by the winds, and consequently the entire "torsional force" exerted by the winds on the earth must, at any given time, be equal in the easterly and westerly directions.

I have now described in outline what theory shows that the circulation of the atmosphere would be in the absence of watery vapour and in the presence of the sun's heat and the earth's rotation; and observation shows that such is the actual circulation on the large scale, and not taking account of local disturbances.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, February 23

Halo round Shadow

IT is not uncommon for an observer, when looking at his own shadow on rough ground or turbid water, to see its head surrounded by a halo, of which the brightest part is in contact with the shadow.

This phenomenon has often elicited notice, but as far as I am aware has not before now been explained, nor do those who have mentioned it seem to have observed that its appearance depended on the nature of the surface receiving the shadow.

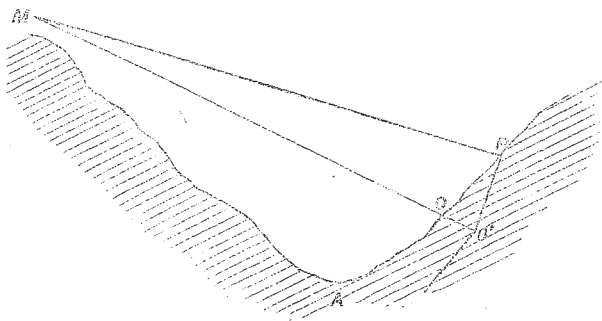
The conditions necessary for the production of these halos are—

1. That the screen, as whatever the shadow is cast on may be called, should not be a continuous surface, but a number of small surfaces with intervals between them, each of these small surfaces of course casting its own shadow on whatever happens to be behind it.

2. That the shadow should be at a considerable distance from the observer.

3. That the light should not fall very obliquely on the screen. The first of these conditions only is essential, but the fulfilment of the last two makes the phenomenon more marked.

Rough grass forms a good screen, especially if, as in the diagram, conditions 2 and 3 are fulfilled by the shadow being



cast on one side of a valley, while the observer is standing on the other.

In the case of the shadow on turbid water, it must be remembered that it is not the surface of the water which forms the screen, but the particles suspended in it.

The general explanation of these halos is this—

From the observer's point of view the screen in the immediate neighbourhood of the shadow of the head is seen in nearly the same direction as it would be from the source of light. In this direction, therefore, each of the small surfaces of which the screen is made up will hide its own shadow, but this will be true of no other direction; and the effect on the whole will be that the screen will appear brighter close to the shadow of the observer's head than elsewhere.

To examine this rather more in detail, let MAO be a section

of the ground passing through the observer at M and his shadow at O . Let

$$\begin{aligned} O'P &= r \quad O'MP = i \\ O'PM &= a \text{ right angle.} \end{aligned}$$

Let w and w' be the projections on $O'P$ of the average breadth of the sections of the small surfaces made by the plane MAO , and the average distance between them respectively, and let h be the average distance of each of the small surfaces from its own shadow.

Then the amount of light received from any space $r d\theta$ ($w + w'$) may, *ceteris paribus*, be taken without any great error as a measure of the brightness of the zone whose mean radius is r , and whose breadth is $w + w'$ ($d\theta$ being a small rotation of r about $O'M$), and this will be proportional to $w + w' - h \sin i$. The decrease in brightness is proportional to h and $\sin i$, and will reach a maximum when $h \sin i = w$, if $w < w'$, or $= w'$ if $w' < w$.

Outside the circle defined by this value of i the brightness will be sensibly constant, because the quantities of which w , w' and h are the average values have all manner of actual values, even in a very small space.

These expressions are only approximate, but they serve, as well as the longer exact formulæ, to show the general laws of the phenomenon.

ARNULPH MALLOCH

Meteor

THIS evening, at close upon twenty minutes past six, as I was walking in my garden towards the almost full moon (which was very bright), I observed a brilliant meteor pass from right to left over, and very near, the moon's disc. It was visible for a distance of about twice her diameter. From the amount of daylight, and the extreme brightness of the moon, I judge this meteor to be worth recording.

C. M. INGLEBY

Valentines, Ilford, February 26

Tape-worm of Rabbits

SO far as I am aware the only evidence in favour of the view that *Bothriocephali* present no hydatid stage is that which has been furnished by the researches of Knoch. To me it has always seemed that this evidence is insufficient fully to overcome the analogical probability that tape-worms of this genus resemble tape-worms of other genera in passing through a hydatid stage—and this notwithstanding the occurrence of a ciliated embryo. However, in my previous letter I ought no doubt to have alluded to the researches of Knoch, and should certainly have done so had my object in writing been other than it was, *i.e.*, merely to ascertain whether anyone had as yet taken the trouble to trace the life-history of the rabbit's tape-worm.

February 20

GEORGE J. ROMANES

A PROBLEM IN THE NATURAL HISTORY OF THE SALMON.

MR. FRANK BUCKLAND, in giving evidence before the Parliamentary Committee, which during last session of Parliament inquired into the condition of our oyster fisheries, stated that "a salmon (? *Salmo salar*) does not breed every year, but every three years!" On being asked by a member of the Committee if he had any proof of his averment, Mr. Buckland stated that, "he had a great idea of it," but was deficient in proof. Before examining this alleged fact in the life of the salmon, advanced by Mr. Buckland, it is proper that we should state briefly what induced him to make known his idea.

While illustrating the theory of oyster spatting, and telling the Commissioners that all the individual oysters on a *scalp* would not be found exuding their young at the same time, however favourable for spatting the period might be, Mr. Buckland also enunciated his opinion as to the periods at which salmon spawn. That gentleman holds that only one of every six oysters on a *scalp* will be found in a procreant state during the same season; and, by way of clenching his illustration, he said, "you never get salmon always breeding the same year, they take time to recover themselves, and so forth." This latter state-